limits. It would carry on in the Far East the work already performed in British India and Burmah.

A SPANISH Expedition under Capt. Cervera has been exploring Adrar in the Western Sahara. Capt. Cervera describes Port Rio de Oro, where he landed from the Spanish cruiser *Ligera*, as rather difficult of entry, but, once entered, as secure from all winds, with good anchoring ground, and from 10 to 30 metres' depth of water. "Rio de Oro" is a misnomer, as there is only one well of fresh water, and that very dirty. There are, however, good wells in the interior, and at four days' journey there is a running spring. The Expedition proceeded, between latitude 22° and 23°, south-eastwards 425 kilometres through an arid country of gneiss and granite, and struck the boundary of Adrar. The population is composed of four tribes—the Uled Delim, speaking and nearly all capable of writing pure Arabic, mixed with a few words of Berber origin. These tribes are nomadic, moving their tents from well to well for the pasture of their dromedaries, goats, and sheep. The capital of Adrar is Aatar, not Wadan, as hitherto believed. Wadan lies more to the south.

"HYBRID" WHEAT

IT is probably not generally known that the cereal from which we obtain our bread corn is almost invariably self-fertilised in nature, and that only a skilful expert can perform the delicate operation involved in the cross-breeding of wheat. The anthers, when near maturity, must be removed from a number of wheat-flowers, and on the following day the pollen of the male parent must be placed on the stigma. The opening of the glumes, however, is dependent on the swelling of the "lodicules," which only occurs when the temperature of the atmosphere is not less than about 75°. Below that minimum the florets will not open so as to expose the reproductive parts to the operator. The angle of opening of the glumes corresponds to this swelling, and when fertilisation has been performed the lodicules shrivel up and the glumes again close over the pistil. It had long been obvious that half a dozen different varieties of wheat, blossoming at the same time, may be grown in adjacent fields or in contiguous rows without the occurrence of interbreeding, in spite of the clouds of pollen which sunshine and warmth develop at the time of blossoming; and considering the remarkable results from the cross-fertilisation of numerous plants in gardens, it seems surprising that the same process should not have been applied to wheat. Many years ago a well-known selector and "improver" of cereals, the late Mr. Patrick Sheriff, tried some experiments in this direction. His usual method of improvement consisted in the selection and careful cultivation of "sports," and he was approaching the end of his career when his earliest attempts at cross-breeding were made. The increased vigour of wheat, the moulding of the ear, the production of a larger and fuller ear, with superior grain, earlier maturity, and the modification of the straw so as to render it stronger, or shorter, and less liable to become laid as in the present season, are all improvements which may certainly be accomplished in regard to this cereal, just as analogous modifications have beeen effected in animals and some other plants by the recognised methods of breeders.

The wheat-crop of the United States reaches at present 50,000,000 quarters, or four times that of England, and this may in some measure account for the numerous experiments in cross-breeding by scientific American farmers, and especially by some of the professors of agriculture in the colleges of that country. The same remark applies to France, where the cultivation of wheat is relatively far more important than in England, and where the noted seed-firm of Vilmorin are now in the midst of the work of cross-breeding. But even in England, disheartened as farmers may be as regards wheat-culture, their prospects might certainly be improved if the average production of this cereal could be increased, its quality improved, and its liability to disease and injury from indifferent weather diminished. Both growers and consumers, therefore, have an interest in the undertaking of Messrs. Carter and Co., the seedsmen, who for several years past have been engaged in the cross-breeding of wheat at their trial-grounds and gardens at Forest Hill. The collection of different sorts of wheat at this establishment includes varieties from every country which exports this grain to England. Some of them are not hardy, and the wretched appearance of the growing specimens of Persian and Indian varieties was probably due to their depreciation in our climate. Some of the colonial and other sorts were excellent, but none could compare to the so-called hybrids.

The operations commenced in 1882 by the sowing of a number of the best English and American varieties, and in the following summer twenty crosses were effected by experts who are usually employed by the firm in delicate manipulations of a similar kind in connection with garden vegetables and flowers. In the following autumn the hybrids, as they are usually called for convenience, were sown between the rows of the male and female parents for the sake of comparison, and in the succeeding year the mixture of the breeds became apparent. In one plot, for example, the female parent was a short-strawed velvet-chaffed variety, and the male a very large, bearded, and tall American wheat, and the offspring attained a stature exceeding that of the former by a foot, with smooth chaff, and stout thick-set ears bearing minute awns at the apex of the chaff of each grain. This last-named peculiarity, the occurrence of defensive points in serrated order from top to bottom of the ear, may be referred to as one of the many advantageous peculiarities which have been developed in the course of the experiments, and it has gained for the new variety the appropriate name of "Bird-proof."

Another of the cross-breeds, having the earliest of English varieties, Talavera, for one of its parents, was almost ready for cutting this year on July 21, when we inspected the new sorts, a very early date in the case of a late backward harvest. Another has the grains very firmly set, and therefore not liable to shell out even when the crop is dead ripe, as it usually is before the time of cutting in New Zealand, where this wheat will probably prove popular.

Another of the crosses proved to be a wheat with shorter straw than any other variety in cultivation, and this too will prove a valuable modification, since neither soil nor season, however productive of straw they might be in certain years, could throw the crop down. Now does it surprise the experts that the offspring of two parents which are both of average height, should prove to be a dwarf in regard to the length of its straw, since they have had occasion to observe the same thing in the breeding of peas—two sorts of peas, each 4 feet high, and requiring the support of sticks, having produced a very useful seedling of 2½ feet, which requires no such artificial assistance.

We cannot attempt a detailed description of the numerous other peculiarities—some of them promising to be highly advantageous—which have been developed in the course of these wholesale experiments. But we may here observe that the most tiresome part of the business has proved to be the fixing of the types after the crossing had been accomplished. The work, however, has proved sufficiently successful to encourage the experimenters to undertake the cross-breeding of barley as well as wheat, and to lead them to anticipate a large demand for their new varieties, not only in this country, but in the colonies.

H. E.

DR. AUGUST WEISMANN ON THE IMPORTANCE OF SEXUAL REPRODUCTION FOR THE THEORY OF SELECTION[1]

IN NATURE, vol. xxxiii. p. 154, was given an article on Prof. Weismann's most interesting and important memoir on "The Continuity of the Germ-Plasma considered as the Basis of a Theory of Heredity." The present memoir also abounds with interest, and may be regarded as following naturally from the former one as a continuation and further elaboration of some of the questions raised in it. The main aim of the memoir is to establish the position that the process of sexual reproduction is the prime agent by which all the varied differentiations of the complicated phyla of the Metazoa has been brought into existence. A strong part of the argument is devoted to the establishment of the position that peculiarities acquired by the parent are not transmitted to the offspring, and to showing that the hypothesis that such acquired peculiarities are transmitted is not necessary for the explanation of the known phenomena of heredity and the mode of origin of the series of organic forms. It will be remembered that the assumption of this position forms an important and necessary factor in the theory of the continuity of the germ-plasma, but it was one which in the former memoir was only lightly touched upon.

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[1] "Die Bedeutung der sexuellen Fortpflanzung für die Selektions Theorie." (Jena: G. Fischer, 1886.)

A large part of the contents of the present memoir was delivered as a lecture at a meeting of the German Naturalists' Association at Strasburg in September 1885; but numerous alterations and additions have been made, and six appendixes on special points have been added.

At the outset, special attention is drawn to the essential difference between "those special new characters which are correctly to be included under the term 'acquired,'" and the much broader class of new characters generally. Only those new characters can be termed "acquired" the origin of which is due to external influences, and not those which depend on the mysterious collaboration of the different tendencies of heredity which meet one another in the impregnated germ. These latter are not acquired, but "inherited." If, as the author holds, acquired characters are incapable of being transmitted to the progeny by the parent, then a much wider field of action must be ascribed to the processes of selection in the transformation of species than hitherto, since the modifying influences of external conditions being confined to the individual, can, in the vast majority of instances, have no effect on the transformation of species.

After discussing Nägeli's postulation of the existence in organisms of a special internal modifying force to account for the phenomena of adaptation, and showing that such a force has no existence at all, and that there are no reasons or justification for assuming it, the author dwells on the adequacy of the theory of selection to account for the facts. He insists specially on the necessity that the changes occurring during the transformation of a species, both in the organism itself and in the conditions of existence, shall take place by the smallest possible stages with the utmost slowness, so that at no moment of the entire process of transformation shall the species remain inadequately adapted to its conditions.

The possibility of the transmission of acquired characters being excluded, it is assumed that, in the case of all animals and plants which are reproduced by real germs, only those characters can be transmitted to a succeeding generation which were already present in the germ at the time of its formation.

On the theory of the continuity of the germ-plasma, a certain minimum of the active substance of the germ, the germ-plasma, always remains unchanged when the germ develops into the organism, and this remains of the germ-plasma becomes the basis from which the germ-cells of the new organism are formed. Thus there is a continuity of germ-plasma from one generation to another, and the impossibility of the transmission of acquired characters follows directly from this position, since the molecular structure of the germ-plasma is already determined within the embryo.

There are no facts which really prove that acquired characters can be inherited, although many attempts have been made to render such a supposition plausible. On the contrary, the evidence against it is abundant. The children of highly-civilised races of mankind, when brought up in isolation, show no trace of a language, and the results of experiments made on plants tell markedly on the same side.

Twenty-two pages of appendixes are devoted to demonstrating this most important position—the influence of which on future speculation with regard to organic evolution can hardly be over-estimated—that acquired peculiarities are not hereditarily transmissible. Only some of the statements with regard to the familiar instance of Brown-Séquard's hereditarily epileptic guinea-pigs can here be given.

Brown-Séquard, as is well known, produced artificially epilepsy in guinea-pigs by means of section of certain parts of the central or even peripheral nervous system in the living healthy animals. The progeny of these guinea-pigs inherited the disease of the parents. The experiments were repeated by Obersteiner, and there is no doubt of the fact. Still, urges our author, this is not to be taken as a proof that acquired peculiarities can be inherited. Epilepsy is no morphological peculiarity, but a disease.

If there were found in the epileptic offspring a distinct and evident morphological alteration in the nervous structures caused by and corresponding with that produced by injury in the parent, and which was at the same time the cause of the epilepsy in the offspring, then the question of the actual transmission of an acquired morphological peculiarity might be justly raised.

That such is the case is, however, not only not proved, but very improbable. What is certain is that many of the young of such artificially epileptic parents are small and feeble, and often die early; others show deformities and sores on various parts of their bodies. In rare instances certain of these young exhibit epileptic symptoms. Two only out of thirty-two young of epileptic parents showed the symptoms, and these soon died, having but very little vitality.

The facts may be fairly expressed by stating that the guinea-pigs artificially rendered epileptic transmit to a part of their offspring the disposition to various diseases of the nerves, to diseases of the motor nerves, and in a less degree to those of sensory nerves, and most markedly to those of trophic nerves. In rare instances, those in which paralysis is most developed in the offspring, the epilepsy is also transmitted with it.

If the pathological change in the nervous structures which follows injury and produces the epilepsy be due, as is possible, to some as yet unknown microbe growing within its substance, it is far easier to understand the transmission of such a microbe from the parent to the offspring in the adult sperm-cell or ovi-cell than to conceive of the disease being communicated in the form of a molecular change in the germ-cell. That such a transference of a microbe in the ovi-cell or sperm-cell occurs in the case of syphilis and tuberculosis is probable, and it is certain that such does occur in the case of the muscardine silkworm disease. Such an explanation would account, in the case of the guinea-pigs, for the fact that the various offspring exhibit various forms of nerve diseases, which remains unexplained if it be assumed that this is a case in which there is an hereditary transmission of a morphological character, namely, a pathological change of structure of a nervous centre. The way in which the artificial epilepsy develops itself in the guinea-pigs after the operation, creeping gradually over the body, and ensuing in the same way after injury to the most varied parts of the nervous organs, is a proof of its infectious nature. The change produced by section of the nerves is obviously not the direct cause of the epilepsy, but only serves to originate a process of disease which is propagated centripetally, with the final result of the appearance of epileptic symptoms.

The germ-plasma is immensely complex in its finest structure, but it has a remarkable power of persistence, since it absorbs nourishment and grows enormously without in the least changing its complicated molecular structure in so doing. This follows from the fact that many species (*e.g.* the Egyptian ibis or the crocodile) have reproduced themselves for thousands of years without change. Further, since the quantity of germ-plasma contained in the single germ-cell must be regarded as extremely small, and as only a minute fraction of this remains unchanged when the germ-cell becomes developed into the new individual, the growth of this fraction in the individual must be a most enormous one, as usually thousands of germ-cells are produced by it.

Since the germ-plasma can remain unchanged in molecular structure in spite of such vast increase in bulk, it is obvious that it is not easily to be modified, and it is probable that the direct influence of modifications in surrounding conditions on the germ-plasma has no effect of any importance on the production of hereditary individual variations. These must have another origin, and this, according to Weismann, is to be sought in the mode of reproduction occurring amongst most organisms at present existing—sexual, or as Haeckel terms it, amphigonic reproduction. This consists, as is well known, in the fusion of two antithetic germ-cells, or possibly nuclei only, which contain the germ-plasma, which in virtue of its peculiar molecular structure, is the bearer of the hereditable tendencies of the organism from which the germ-cell originates. Thus in amphigonic reproduction two different sets of hereditable tendencies are to a certain extent mingled with one another. It is this process of mingling which is the cause of the occurrence of hereditarily transmissible individual peculiarities, and it is the production of these peculiarities which it is the office of amphigonic reproduction to effect. Amphigonic reproduction supplies the material for the development of individual variation out of which selection produces new species.

This is a most startling conclusion, and directly opposed to almost all former views on the action of sexual reproduction. The amphigonic process has been regarded as having the effect of rapidly obliterating any deviations from the specific type which may arise in the members of a species. With regard to specific characteristics this may still hold true, since the deviations from them are of so rare occurrence that they are unable to hold their own against the great mass of normally-formed individuals. But it is otherwise in the case of the minute differences which are characteristic of the individual, because every individual possesses them, and each in a different manner. Here a compensation of the differences could occur only if the entire species were composed of but few individuals, but the number of individuals which constitute a species is in most cases practically unlimited, and thus an intercrossing of all of these with all the others with the result of the compensation of the individual differences is impossible.

The process of amphigonic reproduction is able to bring about what is absolutely essential to the working of selection as an all-powerful agent in development, namely, the summing up of the small individual differences it has to deal with in the direction of the result aimed at, and the production by this means of new characters. Such summing up of minute characters is impossible in the case of species with non-sexual reproduction. In the course of successive sexual generations the differences between the individuals of a species must continually increase, not as regards the greater differences, but as regards the constantly new combinations of individual peculiarities which are formed. Imagine a number of individuals of a species distinguished each from the others by a few hereditable peculiarities; in the next generation no single individual could be like the others, they must all be different. Further, not one of the progeny could be identical with one of their parents, since each has combined in it the hereditable tendencies of two parents, its organism being as it were a compromise between the developmental tendencies of both. In the third generation the hereditary tendencies of two individuals of the second generation are combined. But since the germ-plasma is no longer simple, but is already compounded of two individual kinds of germ-plasma, an individual of the third generation will represent a compromise between four different hereditable tendencies. In the fourth generation eight such tendencies must be combined; in the fifth, sixteen; in the sixth, thirty-two. Each of these tendencies may show its effect more or less marked in this or that part of the organism as it is developed, and thus in the sixth generation a number of the most different combinations of the individual peculiarities of the ancestors may be exhibited; combinations such as never before existed in the history of the species, and such as never can occur again.

The prepotency of the various kinds of idioplasma which compose the germ-plasma of the germ-cells of each individual must vary in intensity at various periods of its life. This must be assumed in order to account for the fact that the several children of the same parents are never exactly alike. The presence of the small individual peculiarities postulated as displayed in the hypothetical series of individuals of the first generation is accounted for as having arisen, not amongst the higher organisms, the Metazoa, but amongst their unicellular Protozoan ancestors. Amongst these there is no distinction between body-cells and germ-cells; their reproduction is by division, and in this case every modification of the body of the organism, every individual peculiarity, however produced during the course of life, must be directly transmitted to the offspring. Here parent and child are in a certain sense still one and the same being. The child is a portion of the parent, and commonly half the parent. The conditions are entirely different from those occurring in the sexual reproduction of the Metazoa, by which acquired peculiarities are not transmitted.

Hereditarily transmissible variation having arisen in the Protozoa by the direct action of external causes, was retained by the Metazoa when they became developed; and as amphigonic sexual reproduction arose at the same time, the variation became thereby enhanced and increased in complication, and preserved in ever-changing combinations.

In the theory of sexual reproduction put forward by Prof. W. K. Brooks, of Baltimore, in 1883,[1] there is a resemblance to the views here maintained, in that sexual reproduction in it also is regarded as the means which is employed by Nature to produce variations, but the mode in which the influence is supposed to act is entirely different in the two theories, Brooks attributing the main effects in variation to the inheritance of acquired peculiarities, and to what may be termed the "circulation of the germ-plasma." His theory is a modification of Darwin's "pangenesis." He assumes, like Darwin, that each cell of the body of higher organisms throws off minute gemmules, not always and under all conditions, but only when they encounter unusual circumstances of existence. Whenever the normal functions of any component cell of the body are disturbed, "it throws off small particles which are the germs or gemmules of this particular cell." These gemmules are, according to Brooks, capable of passing to all parts of the body, and may pass into an ovarian ovum or a bud, but the male germ-cell has a special attractive force for them, gathering them within itself and storing them up. It is one of the peculiar merits of the theory of the continuity of the protoplasm that it dispenses with all necessity for any hypothesis of a circulation of gemmules or germ-plasma, so complicated and difficult to understand as these hypotheses are. As soon as the inheritance of acquired peculiarities is denied, such hypotheses are not required. Brooks further differs widely from Weismann in ascribing to the two germ-cells concerned in impregnation a different *rôle* in the process, the egg-cell or conservative principle being supposed to be charged with comparatively few gemmules, the sperm-cell, or radical principle, with many. This view, that the male germ-cell has a different part to play during the construction of the embryo from the female germ-cell is considered by Weismann to be untenable, because it is in contradiction with the simple matter of observation that human children on the whole are able to inherit just as many characteristics from the mother as from the father.

Apparently, the only function of amphigonic reproduction is the production of a supply of hereditable individual peculiarities on which selection may work, and as the development of the whole higher organic world depends on these processes, the part which amphigony has to play in Nature is one of the most stupendous conceivable, and hence its wide, almost universal, distribution in the animal and vegetable kingdoms. Nevertheless, it is not in any way contended that amphigonic reproduction originally came into existence in order to render the production of species possible. The process must have been already present before it evoked the hereditable variability, and its first appearance must therefore have had another cause. This cannot at present be explained with any certainty, but the key to the problem lies in the conjugation of the Protozoa, the predecessor of amphigonic generation. The fusion of two unicellular individuals into one, the simplest form of conjugation, must have a direct and immediate action which is of advantage to the existence of the species concerned. The view taken by Hensen and E. Van Beneden and others, that conjugation, as well as sexual reproduction generally, effects a rejuvenescence of the organism, is discussed and rejected as unsatisfactory. "The entire conception of rejuvenescence has something indefinite and nebulous about it. The assumption of the necessity of a rejuvenescence of life, brilliant as it is, is scarcely to be reconciled with our other conceptions of the nature of life based on purely physical and mechanical forces. How can it be conceived that an infusorian which has at last by repeated division exhausted its power of reproduction can recover this power by fusion with another similarly effete infusorian? Twice nothing does not make one, and if it be assumed that each infusorian retains half its reproductive force, the two combined would result only in one whole, but this could hardly be termed rejuvenescence. It amounts only to an addition such as, under other conditions, would be produced by simple growth."

It is best to assume in the present state of knowledge that living matter is endowed with a power of unlimited assimilation and consequently unlimited capability of reproduction, and that the form of reproduction, whether sexual or asexual, of itself exercises no influence on the duration of the process. It has not been proved that reproduction by division can never take place indefinitely. The phenomenon of parthenogenesis is strongly against the hypothesis of rejuvenescence, for, if impregnation represents a rejuvenescence, and essentially consists in a combination of energies and materials which in virtue of their differences give rise to the development of reproductive force, it is difficult to understand how occasionally the same reproductive force can be produced by one of the two materials, only the egg-cell. The common assumption that in the case of parthenogenesis a single impregnation suffices for a whole series of generations has no grounds of support, and is at variance with the fact that the same egg which may develop parthenogenetically (in the queen bee) is also capable of fertilisation.

The primitive action and meaning of conjugation may at present be best provisionally defined as a strengthening of the forces of the organism in relation with reproduction, which occurs when, on account of external causes, such as atmosphere, temperature, and want of food, the growth of the single animal to the size necessary for reproduction is not possible. This cannot be regarded as equivalent to rejuvenescence, since rejuvenescence is a process necessary for the maintenance of reproduction, and ought to occur periodically entirely independently of external conditions, whilst according to the above view conjugation originally only presented itself under unfavourable conditions of life and helped the species to surmount them.

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[1] W. K. Brooks, "The Law of Heredity: a Study of the Cause of Variation and the Origin of Living Organisms" (Baltimore, 1883).

Amongst the higher Protozoa the original import of conjugation seems already to have dropped into the background, as shown in the change in the nature of the process itself. The higher Infusoria are only temporarily fused with one another in conjugation, and it appears possible, and even probable, that the process has here already attained the full significance of sexual reproduction, and is to be regarded as functional as a source of variability only.

Amphigonic reproduction, having existed through countless generations of Protozoa in the form of conjugation, passed over to the Metazoa, and, though its original physiological effect lost importance or retired altogether into the background, was preserved from extinction and firmly retained because of the immeasurable advantages which are conferred by it in endowing the species with the power of adapting itself to new conditions of existence. The formation of new species which was possible amongst the lower Protozoa even without amphigony, amongst the Metazoa and Metaphyta was to be attained only by that process.

Amphigony has been lost in certain cases, either partially, as in the case of some lower Crustacea amongst which parthenogenetic generations alternate with sexual, or entirely, as in the case of certain gall-insects and plant-lice amongst which parthenogenesis has become the only form of reproduction. Such restriction to parthenogenetic reproduction may act so as to secure the existence of a species for a time, but according to the views as to the origin of hereditary variability here maintained, such a species dependent on parthenogenesis alone for reproduction must be near its period of dying out, as unable to adapt itself to any new conditions of existence which may arise, since in the loss of amphigonic reproduction it has lost the capability of mingling and increasing the individual hereditable peculiarities which occur amongst its members.

This conclusion is supported by the fact that no whole groups or genera occur the species of which are entirely parthenogenetic in their reproduction.

The persistence of functionless organs in species which are reproduced parthenogenetically is a further corroboration of the general view as to the import of amphigonic reproduction here maintained. Since acquired peculiarities are not inherited, organs which fall out of use cannot become vestiges in a direct way, as has been hitherto assumed to be the case. The functionless organ becomes indeed weaker and less fully developed in the individual which does not use it, but this reduction in the organ is not transmitted to the offspring.

The explanation of the undoubted fact that such organs do become vestiges must be sought elsewhere. In order that any particular part of the body in any species may be maintained at the height of its functions, all individuals which possess this part in less completely perfect development must be excluded from participation in the act of reproduction by perishing in the struggle for existence. As soon, however, as an organ ceases to be useful, this uninterrupted selection of the individuals with the best organs for the purpose of reproduction ceases also, and a condition arises which the author terms "panmixia." Now not only the individuals with the best organs participate in reproduction, but also those with inferior ones. A mixture of all kinds of gradations in goodness and badness in the organ must be the inevitable result, and thus in the course of time a universal deterioration in the organ must be produced. The remarkable fact that the gradual disappearance of functionless organs is extremely slow appears much more in keeping with the above views as to the nature of the process than with those hitherto adopted. The effect of disease of an organ in the course of a single life is a very well marked one, and if it were transmissible even to a reduced extent direct to the offspring, the organ must become reduced to a minimum even in a hundred generations. Yet how many million generations must have elapsed since the whalebone whale abandoned the use of its teeth?

If this new view of the cause of the reduction of disused organs is assumed as correct, it follows that vestiges of organs can occur only amongst species with amphigonic reproduction, not amongst those with parthenogenetic reproduction only, and this appears to be the case. Superfluous organs do not become rudimentary in species parthenogenetically reproduced. As far as the author's investigations extend, the receptaculum seminis does not become aborted in such species, although it is entirely without function. Thus in Chermes, which is without males, the receptaculum seminis is present in the females unchanged; whilst, on the other hand, in Aphis, another plant-louse in which amphigonic reproduction is not extinct, but alternates regularly with parthenogenesis, the receptaculum seminis has become lost in the summer female.

These evidences in favour of the general views here expressed form, of course, no absolute proof of their correctness, but only give evidence in favour of their probability. Further evidence cannot be offered at present, the phenomena dealt with being extremely complicated and their explanation being such as can only be approached gradually.

The author, however, considers that he has plainly shown that the selection theory is by no means incompatible with the conception of the "continuity of the germ-plasma," and further, that as soon as this conception is accepted as correct sexual reproduction appears in an entirely new light, displays a reason for its existence, and becomes to a certain extent comprehensible.

H. N. MOSELEY

N.B. A criticism of Prof. Weismann's above theories by Prof. von Kolliker appears in the *Zeitschrift für Wissenschaftliche Zoologie*, in the October part just issued.

THE FUNCTION OF A UNIVERSITY[1]

I GRATEFULLY accept the honour with which I have been invested thus publicly, and with such kindly feeling. In the future, as in the past, I shall give you my best endeavours.

By a fortunate accident it has happened that I am not called upon to speak to you on behalf of the University of California before I know somewhat of it. The six months which have elapsed since I entered upon the duties of the high office which I now hold, I have utilised in studying with minute care the University in its main features and in some of its details. This is not the place nor the time to speak of minor matters. It is, however, the very place and the very time for me to say to this audience that no friend of the University has any cause to fear, so long as all of us, Regents, Professors, and students alike, remain united in the future, as we are at this present moment, in a single effort toward the same high ends.

For nearly a hundred years the American Republic lived, and grew, and prospered, and the community of nations hardly knew her, and barely gave her place. So, on a smaller scale, it has been in California. The University here has lived, and grown, and prospered, and the communities outside of our own small circle hardly know us, and grant us our place reluctantly. It is a perfectly safe prediction that within the next twenty years, possibly within the next ten, the State of California will find suddenly that here in her midst she has a force on which she never reckoned—a reserve on which she never counted.

It is easy to see what advantages would come if this conviction were now wide-spread and firmly held. It is easy to seek feverishly to make ourselves quickly known, in order that we may be more widely useful. But, I am more and more convinced that if we are always ready, like a strong man, our opportunity will be here almost before we realise it.

Vital Points and Fundamental Principles.—What, then, are the aims upon which our eyes must be ever fixed, and toward which our energies must be ever directed? I will not name them all, nor count them over one by one. But I think that I can point out certain vital points that must be guarded; certain principles that must be fundamental. Let us consider the demands which the community makes of the University, and again, the standards which the University should set for its individual members.

In the first place we must carefully examine what it is that we, as citizens, demand from the University. We must see to it that what we demand is consonant with what we ought to demand. If we find that we disagree with what seems to us to be the position of the University, ought we not in fairness to calmly inquire which of the two is right? Is there not at least a certain presumption that the efforts of a body of intelligent scholars cannot be all misdirected? The usual and careless way of meeting this question, even on the part of those who count

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[1] Inaugural Address by President Holden at the University of California.